XIX. *A short Account of some Observations made with Chronometers, in two Expeditions sent out by the Admiralty, at the recommendation of the Board of Longitude, for ascertaining the Longitude of Madeira and of Falmouth. In a Letter to* THOMAS YOUNG, *M. D. For. Sec. R. S. and Secretary to the Board of Longitude. By Dr.* JOHN LEWIS TIARKS.

Read April 29, 1824.

SIR,

AGREEABLY to the wish of the Board of Longitude, communicated to me through you, I beg to transmit to you the following short statement of the results of chronometrical observations, which were made by order of that Board in the year 1822 and 1823, chiefly regarding the longitude of places in England. I have compared the results thereby obtained, with the corresponding ones contained in the Account of the Trigonometrical Survey of England, published in several volumes of the Philosophical Transactions, and have added some remarks on the method employed in calculating the results of that Survey, with a view to explain the cause of the errors in the longitude of all stations, to which it seems to have given rise. In the year 1822, being ordered to determine the difference of longitude between the island of Madeira and Falmouth, I took fifteen chronometers from Greenwich to Falmouth by sea, and after having performed a voyage to Madeira, carried them back from Falmouth to Greenwich. Both before and after each of the two voyages between Greenwich and Falmouth, the time and the rates of the

chronometers were accurately determined at Greenwich by the Astronomer Royal, and at Falmouth by myself; and applying the means of the rates of each chronometer before and after each voyage, as the rate during the voyage, the means of all results nearly agreed in giving the longitude of Pendennis Castle (the station near Falmouth) 4″ (time) greater than it is laid down in the Account of the Trigonometrical Survey. Although it thus became very probable that some error had crept into the determinations deduced from the Survey, still the result of the chronometers, considering the manner in which it was obtained, could not be looked upon as completely adequate to prove even the existence, and much less the amount of an error, which was before so little expected. As this question affected however the longitude of all places in England determined in the same manner as Pendennis Castle, and likewise that of the island of Madeira, the difference of longitude of which from Falmouth had been ascertained, it was resolved to determine more accurately, by means of chronometers, the difference of longitude between Falmouth and Dover, the latter being a station in the Survey easily accessible by sea, and its difference of longitude from the former nearly the greatest possible in England, viz. more than $6^\circ\frac{1}{3}$. In pursuance of the method adopted for this purpose, the chronometers were constantly transported from the one place to the other, as soon as the time at the former was determined with sufficient accuracy. It is clear that in this manner there may be deduced from each chronometer two sets of numbers, one for each place, representing the difference of the time of the chronometer from the mean time of the place at successive moments, and

that between any two successive terms of one set, a term corresponding in time to one of the other set may be determined by interpolation, which will represent the difference of the chronometer from the mean time of one place at the same moment at which the difference from the mean time of the other place was determined by actual observation. The difference of two such terms of both sets answering to the same moment being the difference of longitude of the two places, it follows, that the number of results thus obtained, is always equal to the sum of the numbers of terms in both sets, *minus* two, or to the number of trips from one place to the other, *minus* one. The chronometers which were employed on the occasion were carefully placed on board ship, and never removed from their places during the whole time of the expedition; and no rate was therefore used which was determined while they were in another situation; an advantage, it appears to me, of some consequence. The time was determined by numerous equal altitudes of the sun, taken with a sextant, for which two assistants carefully noted down the moments of observation, by two chronometers; the differences of which from the other chronometers were ascertained by several comparisons before and after taking the observations on shore. The number of observations being generally sufficiently great to destroy, in the mean of them, the small errors of each observation, and the rates of the chronometers nearly uniform during the time which it was necessary to stay on shore, the two chronometers usually gave nearly equal results, the mean of which was afterwards adopted in deducing the state of the other chronometers. When the advanced state of the season rendered it dangerous to ride at

anchor in Dover roads, it was thought useful to connect Portsmouth with Falmouth in the same manner. The following are the days on which the observations for ascertaining time were taken, from which the results were derived: 1823. 1. July 30, Dover. 2. August 4, Falmouth. 3. August 6, Dover. 4. August 11, Falmouth. 5. August 6, Dover. 6. August 24, Portsmouth. 7. August 30, Falmouth. 8. September 2, Portsmouth. 9. September 7, Falmouth. 10. September 11, Portsmouth. The difference of each chronometer from the mean time of the respective places being therefore known on these days, there may be employed two modes of calculating the difference for an intermediate moment, viz. the rate between every two successive differences belonging to the same place may be considered as uniform, and the intermediate term is accordingly found by simple proportion from two terms only; or the intermediate term may be derived by interpolation from all the terms, provided that in the whole interval no external cause have acted on the chronometers so as to produce a sudden change in their rates. Thus if d', d'', d''' represent the difference from the mean time of the same place at the moments t', t'', t''', this difference will be for the moment $t = \frac{(t-t'')(t-t''')}{(t'-t'')(t'-t''')} d' + \frac{(t-t')(t-t''')}{(t''-t')(t''-t''')} d'' + \frac{(t-t')(t-t'')}{(t'''-t')(t'''-t'')} d'''$. Each of the above days, except the first and last, respectively combined with two, or all the days belonging to the other place, will therefore give a result. Having thus explained the method which I have used, it will be sufficient to give the mean results of the chronometers, out of which one only was rejected on account of its irregular rate.

Difference of Longitude between Dover and Falmouth, by twenty-four Chronometers.

First Mode of Interpolation.		Second Mode of Interpolation.	
Observations.	Mean Results.	Observations.	Mean Results.
	h ′ ″		h ′ ″
2 and 1.3	0 25 28.351	2 and all Dover Obs.	0 25 28.283
3 and 2.4	28.436	3 and all Falmouth Obs. . .	28.722
4 and 3.5	29.152	4 and all Dover Obs. . . .	28.942
5 and 4.7	27.104	5 and all Falmouth Obs. . .	28.346
Mean . . .	0 25 28.261	Mean	0 25 28.573

Difference of Longitude between Portsmouth and Falmouth, by twenty-six Chronometers.

First Mode of Interpolation.		Second Mode of Interpolation.	
Observations.	Mean Results.	Observations.	Mean Results.
	h ′ ″		h ′ ″
6 and 4.7	0 15 42.560	6 and all Falmouth Obs.	0 15 43.680
7 and 6.8	45.845	7 and all Portsmouth Obs. . .	46.362
8 and 7.9	46.710	8 and all Falmouth Obs. . .	46.731
9 and 8.10	45.534	9 and all Portsmouth Obs. .	46.622
Mean . . .	0 15 45.162	Mean	0 15 45.849

Taking the mean of all results, we have finally for the difference of longitude between Dover and Falmouth $0^h\ 25'\ 28''.417$. The longitudes of the Survey, (applying the reduction to my station at Dover, which was nearly $0''.1$ east of that of the Survey) will give the same difference $0^h\ 25'\ 23''.5$. The difference of longitude of Portsmouth Observatory, near which I observed, and Falmouth, is by the Survey $0^h\ 15'\ 43''.0$. Although the observations from which the above given results for the same difference of longitude are derived, were made under less favourable circumstances and at greater intervals, (especially the first) and therefore do not agree so

well among themselves, still they prove that the result of the Survey is about $3''$ too small; or that it is nearly in the same proportion too small, in which the difference of longitude between Dover and Falmouth, and likewise by the observations of the year 1822, that between Greenwich and Falmouth were found to be deficient. We may therefore safely infer, that it is a general and proportionate defect of all longitudes deduced from the Survey, and not the erroneous longitude of any particular station, which has caused the disagreement between the results of the chronometers and of the Survey. Supposing the final result above found to be correct, the increase of the longitude of the Survey $= 25' 23''.5$, is $4''.92$, and at this rate all the longitudes contained in the Account of the Survey must therefore be increased. Applying this correction, we shall have,

the longitude of Dover (at the Station of the Survey) . .	$0^h\ 5'\ 17''.52$ E
Portsmouth (at the Station of the Observatory) .	4 24 .75 W
Pendennis Castle (at the Station of the Flag Staff)	20 10 .81
In the year 1822 I found the latter, by going from Greenwich to Falmouth .	20 11 .49
and by returning from Falmouth to Greenwich	20 11 13

The difference of longitude between Falmouth and Madeira was found, by the mean of the results of seventeen chronometers, to be $0^h\ 47' 28''.21$. The extremes of these results differ indeed $20''$, but as 9 of them, the mean of which is $0^h\ 47' 28''.23$, do not differ more than $3''.71$ from one another, it is to be presumed, that the above given result is not far from the truth. The station at Madeira (the garden of the British Consul in the town of Funchal) is therefore in longitude $1^h\ 7' 39''.02$ W of Greenwich.

Having now fully proved the errors of the longitudes of the Survey, and likewise shown in what manner they are to be corrected, I conceive it to be of some interest to investigate the cause of the mistake into which those distinguished men have fallen, who conducted this great national undertaking with so much ability and perseverance. The safest and most obvious method of reducing the results of a survey of a country with respect to longitude and latitude, would be (if practicable) to determine astronomically, in the country itself, arcs both of the meridian and of a parallel. The spheroid nearest approaching to the figure of the earth for that country, would be easily deduced from these measurements, and all the reductions would be perfectly correct; but as the determination of an arc of longitude is exceedingly difficult, this method has hardly ever been practised. An arc of the meridian is more easily determined, and the reductions with respect to latitude are readily obtained. In order to find, however, in the most correct manner the arcs of parallels of latitude, it is necessary to combine an arc of a meridian measured in the country, with another measured in the same hemisphere as different from it as possible, in order to determine the eccentricity of the meridians, and the dimensions of the corresponding spheroid. The measurements near the Equator and near the Pole may thus be combined with the arcs measured in several parts of Europe, and the errors in the longitude, arising from the adoption of a spheroid thus determined, will be very small. In this manner BOUGUER's degree near the Equator and the degree of the meridian in the middle between Greenwich and Paris, as they are given in the account of the Survey, would have led to a spheroid of the compression $\frac{1}{295}$,

and all the reductions would have been nearly correct both in latitude and longitude. But instead of proceeding in this manner, it seems to have been the intention to determine, independently of any hypothesis respecting the figure of the earth, from a line, the length of which was ascertained by geodetical measurement, the length of a degree perpendicular to the meridian, in the same latitude in which the length of the arcs of the meridian was known by the distance of the parallels of Greenwich and Paris; an arc of the meridian, and one perpendicular to it, being sufficient to determine the dimensions of the terrestrial spheroid. The line chosen for this purpose is the distance between Dunnose and Beachey Head (DB); its length was ascertained in various ways, all of which gave results nearly approaching one another. As the inclinations of the meridians of Beachy Head and Dunnose (to the line DB) had been observed, and the latitude of the two spots were supposed to be known, both places having been connected by the Survey with Greenwich; it is clear, that in the spheroidical triangle, North Pole, Beachy Head, Dunnose (PBD), the two angles B and D, the sides (PB and PD) and besides the length of the line BD are known. By resolving the spheroidical triangle PBD, the angle P (difference of longitude of B and D) is ascertained. From this and the line DB, the length of the degree perpendicular to the meridian in latitude $50^\circ\ 41'$ nearly, the middle point between Beachey Head and Dunnose is determined; and thence a spheroid, with a compression $\frac{1}{149}$ is found, on which all the reductions have been founded. The latitude of Dunnose, determined by its distance from the parallel of Greenwich, is $50^\circ\ 37'\ 7''.3$, or diminished by $1''.99$ (the correction of the lati-

tude of Greenwich Observatory, which Captain KATER applies as the result of the latest observations, and the use of the French table of refraction 50° 37′ 5″.31. Captain KATER finds this latitude by his observations with the repeating circle 50° 37′ 5.″27, differing only 0″04 from the other. The latitude of Dunnose being therefore correctly deduced by geodetical operations from the latitude of Greenwich, it is to be supposed that the latitude of Beachy Head, the more northern point, deduced by the same operations, has been equally well determined, and at least that there is no considerable error in the difference of latitude of the two places as laid down in the Survey.

In order to understand the method which I have shortly described above, it is to be observed, that the spheroidical triangle PBD could not be resolved from the angles B and D and one of the sides PB and PD only, without assuming a certain ellipticity; but from the two angles and the two sides, the ellipticity may be determined directly; and from this, and the length of the arc of the meridian, the dimensions of the terrestrial spheroid may be found. Introducing therefore the two angles and the two sides into the calculation, as is done in the Survey, is assuming that spheroid for the basis of the calculation, which has its compression determined by the relation between the two angles, the two sides, and the eccentricity of the meridians. For let the eccentrity of the meridian be e, the latititude of $\left.\begin{array}{l}\text{Beachy Head } \varphi' \\ \text{Dunnose } \varphi\end{array}\right\}$ the angles $\left\{\begin{array}{l}B=\alpha' \\ D=\alpha\end{array}\right\}$ and we have by the property of the geodetical line (the shortest line between two points on the terrestrial spheroid) $\frac{\sin.\,\alpha\,.\,\cos.\,\varphi}{\sqrt{(1-e^2\sin.\,\varphi^2)}} = \frac{\sin.\,\alpha'\,\cos.\,\varphi'}{\sqrt{(1-e^2\sin.\,\varphi'^2)}}$, or if ξ', ξ and ψ be determined

by the following equations, tang. $\xi' = \frac{\sin. \alpha^2}{\cos. \varphi'^2}$, tang. $\xi = \frac{\sin. \alpha'^2}{\cos. \varphi^2}$, tang. $\psi^2 = \frac{\sin. (\xi' - \xi)}{\cos. \xi' . \cos. \xi . \sin. (\alpha' + \alpha) . \sin. (\alpha' - \alpha)}$, we have $e = \sin. \psi$, and the compression $= 2 . \sin. \frac{1}{2} \psi^2$.

From e and an arc of the meridian the dimensions of the spheroid are readily found; the angle P, and the length of the geodetical line BD, may likewise be determined. The geodetical line BD, which is used in the Survey to find the degree perpendicular to the meridian, furnishes therefore no new datum for determining the dimensions of the parallels; and this line will vary very little for different values of e, provided the values of α and α' and one of the latitudes be the same, while the other latitude is determined by the above equation. But the value of P will be different for every value of e; and the same geodetical line will therefore correspond to different values of the difference of longitude according to the different compressions which are assumed. It follows therefore from all this, that the dimensions of the parallels have entirely been derived from the arc of the meridian in latitude 50° 41′, and the compression resulting from the values of α, α', φ, φ'. The angle α was observed $= 81° 56' 53''$. $\alpha' = 96° 55' 58''$; φ is according to Captain KATER $= 50° 37' 5''.27$, and the value of φ', resulting from the addition of the difference of latitude of B and D to φ, is $= 50° 44' 21''.67$. This value gives, by the above formula, the compression $\frac{1}{150.5}$, nearly the same as found in the Survey. For the compression $\frac{1}{335}$, the value of φ' would be $50° 44' 20''.35$ for; $\frac{1}{310}$ $\varphi' = 50° 44' 20''.42$; for $\frac{1}{230}$ $\varphi' = 50° 44' 20''.79$. From what was remarked before, it is clear that α and α' being the same, no

compression considerably differing from $\frac{1}{150}$ can be admitted. In order to produce an ellipticity $= \frac{1}{310}$, it would be necessary to diminish both α and α' by $5''.5$. Although it would be difficult to assign the limits of the errors that may have been committed in determining α' and α, still it is not very likely that so great an error should have been made in both places. It is therefore likely that the meridians have, in that part of the country, a greater ellipticity than the whole earth, which would not be surprising, as some of the arcs measured in France appear to indicate even a compression $= \frac{1}{175}$.

From the foregoing observations we may now conclude, that the longitudes laid down in the account of the Survey will deviate from the truth, in the same proportion in which the parallels of latitude on a spheroid, having the degree of the meridian in latitude 50° 41′ equal to that of the earth, and the ellipticity $\frac{1}{149}$ differ from those of the terrestrial spheroid, the compression of which is nearly $\frac{1}{310}$. The following comparisons will further illustrate the subject. If the radius of the Equator be = 3486908 fathoms, and the semi-axis of the earth = 3475560 fathoms, which is nearly the result of the measurements in France, and BOUGUER's in Peru, and corresponds to the compression $\frac{1}{310}$, the length of the degree perpendicular to the meridian in latitude 50° 41′ will be 60975.7 fathoms. For the spheroid adopted in the Survey, that degree is found 61,182 fathoms. The ratio of these numbers is 296 : 297, and the correction of the longitudes would be $\frac{1}{296}$; the same correction is, by the chronometrical

observation, $\frac{1}{309}$. The length of the geodetical line BD, supposing the difference of longitude as determined in the account of the Survey, viz. $1^\circ\ 26'\ 47''.93$, would be 338231 feet; whereas it was found to be 339397.6 feet; but if the longitude be increased in the ratio determined by the chronometers, the line will be 339334 feet, which is only 63.6 feet short of the measurement. The spheroid resulting from the compression which would make the difference of longitude of B and D $= 1^\circ\ 27'\ 4''.75$ (as it ought to be according to the results of the chronometers), and from the degree of the meridian in latitude $50^\circ\ 41'$, viz. 60851 fathoms, would have these dimensions: radius of the Equator = 3487907 fathoms; semi-axis = 3476687 fathoms; compression $\frac{1}{314}$. The results of the chronometrical observations are therefore as much as could be expected in accordance with the correct determinations of the figure of the earth.

I am, Sir,

your most humble and obedient Servant,

J. L. Tiarks.

To Dr. Young,
Secretary of the Board of Longitude.